

EXHIBIT U

**IN THE UNITED STATES DISTRICT COURT
FOR THE NORTHERN DISTRICT OF OKLAHOMA**

State of Oklahoma,)	
)	
Plaintiff,)	Case No.: 4:05-cv-00329-GKF-PJC
)	
v.)	
)	
Tyson Foods, Inc., et al.,)	
)	
Defendants.)	
)	

DECLARATION OF W. MICHAEL HANEMANN, Ph.D.

I, William Michael Hanemann, hereby state as follows:

1. I am a Chancellor's Professor in the Department of Agricultural & Resource Economics and the Goldman School of Public Policy at the University of California, Berkeley.
2. In 1978 I received my Ph.D. in Economics from Harvard University. I have 35 years of experience in the fields of environmental economics, welfare economics, and non-market valuation, including the statistical and econometric analysis of survey data resulting from the application of stated and revealed preference methods to measure economic value.
3. I am one of the authors of the State of Oklahoma's expert report entitled "Natural Resource Damages Associated with Aesthetic and Ecosystem Injuries to Oklahoma's Illinois River System and Tenkiller Lake – Expert Report for State of Oklahoma, in Case No. 05-CV-0329-GKF-SAJ" (hereinafter "Chapman et al. Report").
4. I have reviewed Defendants' expert report of William H. Desvousges, Ph.D, and Gordon C. Rausser, Ph.D, dated March 31, 2009 (hereinafter the "DR Report").
5. Chapter 5 of the DR Report deals with "Biases Resulting from Statistical and Econometric Analysis." Sections 5.1 and 5.3 of that chapter deal with certain statistical and econometric issues arising from the non-parametric estimation of willingness to pay (WTP) that was employed by Chapman et al.
6. I am the co-author of two well-known review chapters and a well-known book on contingent valuation (CV), which describe the non-parametric estimation of CV response data using the method employed by Chapman et al. These are, respectively:

- Hanemann, W. Michael, and Barbara Kanninen. "The Statistical Analysis of Discrete-Response." In *Valuing the Environment Preferences: Theory and Practice of the Contingent Valuation Method in the US, EC and Developing Countries*, edited by Ian Bateman and Ken Willis, Oxford: Oxford University Press, 1999.
- Carson, Richard T., and W. Michael Hanemann, "Contingent Valuation" in Karl-Goran Maler and Jeffrey R. Vincent (eds) *Handbook of Environmental Economics*, Amsterdam: North-Holland 2006.
- Ian Bateman, Richard T. Carson, Brett Day, Michael Hanemann, Nick Hanley, Brett Day, Michael Jones-Lee, Graham Loomes, Susana Mourato, Ece Ozdemiroglu, David Pearce, Robert Sugden and John Swanson, *Economic Valuation with Stated Preference Techniques: A Manual*. Edward Elgar, 2002.

7. The CV survey by Chapman et al. presented respondents with a tradeoff: they could vote to pay a specific additional one-time tax for a program to accelerate the recovery of the Illinois River system and Tenkiller Lake back to what they were like in around 1960. Or, they could vote against the program, in which case their household would *not* have to pay an additional one-time tax, and the river and lake would return *more slowly* to what they were like in around 1960. The specific dollar amount of the tax differed among different versions of the survey – each respondent was told one specific amount, but the amount varied across respondents. The variation in the amount of the tax and the corresponding variation in the responses to the tradeoff question form the basis for the statistical estimation of willingness to pay. In effect, the Yes/No response is regressed on the dollar amount of payment (“the bid amount”). The resulting graph relating the proportion of Yes responses to the dollar amount of the bid traces out the cumulative distribution function for the underlying probability distribution of willingness to pay values in the population being sampled.

8. As explained by Chapman et al. (page 7-3):

“Estimating a cumulative distribution from proportions of binary (i.e., “for” or “against”) responses to a stimulus variable – in this case the monetary payment amount – is a well established problem in statistics.¹ Two statistical methods, parametric and non-parametric, are commonly used to estimate cumulative distributions. With parametric estimation, the researcher postulates a specific mathematical form for the probability distribution, such as the normal or the logistic distribution. Parametric estimation simplifies the problem to one of estimating only the parameters of the specified distribution, e.g., the mean and the variance. The numerical value of the parameters is determined by an estimation procedure such as the maximum likelihood method. Non-parametric estimation is a more general approach that avoids specifying a mathematical form for the probability distribution; instead, the form of the distribution and its parameters are both estimated. Non-parametric estimation, therefore, places minimal assumptions on the probability distribution.”

¹ Typical early applications were to data from dose-response experiments in medicine and related fields. The stimulus was the dosage of a poison (or, conversely, the level of treatment) and the response was whether or not the animal succumbed to the poison, or the person responded to the treatment.

“The only restriction on the estimation is that a cumulative distribution function is monotone; in our context, this implies that the probability of voting “for” should not increase with an increase in the dollar amount presented. In practice, with finite data sets, this restriction is often not satisfied empirically. Therefore it is often necessary to impose this restriction on the estimation. With a parametric approach, the restriction is imposed automatically. A maximum likelihood technique for imposing restrictions to nonparametric estimations was developed by Ayer et al. (1955) and is known as the ABERS estimator (Morgan, 1992; Robertson et al., 1988). The ABERS technique was applied to our data and the resulting estimate of the proportion of votes for the program is shown in Figure 7.1 and Table 7.1.”²

The DR Report concurs that non-parametric estimation is the preferred approach (p. 91, footnote 55): “These nonparametric approaches are more reliable than parametric estimators because they avoid assumptions regarding the distribution of WTP between bid amounts.”

9. In the statistical literature, maximum likelihood is the gold standard for estimation of a statistical model. Non-parametric maximum likelihood estimation has the advantage of robustness mentioned above. Thus, monotonicity-constrained non-parametric maximum likelihood is the preferred method of estimation.

10. The ABERS estimator is well known to be *the* monotonicity-constrained maximum likelihood estimator of a binary response model.³ It is widely referenced in the statistical literature – for example in the monographs by Morgan,⁴ Barlow et al.,⁵ and Robertson et al.⁶ It is also referred to as the “Pool Adjacent Violators” (PAV) Algorithm, because that is what the estimator does. The Morgan book, for example, describes the estimator on pages 304-305 with an example in the table on page 305. Barlow et al. describe the same estimator on pages 13-14. Robertson et al. describe this estimator on pages 8-10. In the econometrics literature, the same estimator is referred to by Coslett⁷ and McFadden,⁸ among others. For example, on page 772, Coslett describes a pooling procedure identical to that described by Morgan and the others. McFadden describes a pooling procedure in the bottom paragraph of the right-hand column on page 701, which is identical to that described by Morgan and the others.

² The first reference is to: Ayer, M., H.D. Brunk, G.M. Ewing, W.T. Reid, and E. Silverman. 1955. An empirical distribution function for sampling with incomplete information. *Annals of Mathematical Statistics* 26:641-647. The other two references are given below.

³ The statistical model used to analyze single-bounded CV responses, such as were collected by Chapman et al., is an example of a binary response model.

⁴ Morgan, B.J.T. 1992. *Analysis of Quantal Response Data*. Chapman and Hall, New York..

⁵ Barlow, R. E., D. J. Bartholomew, J. M. Bremner, and H. D. Brunk. 1972. *Statistical Inference under Order Restrictions: The Theory and Application of Isotonic Regression*. John Wiley, New York.

⁶ Robertson, T., F.T. Wright, and R.L. Dykstra. 1988. *Order Restricted Statistical Inference*. John Wiley, New York.

⁷ Coslett, Stephen R., “Distribution-Free Maximum Likelihood Estimator of the Binary Choice Model,” *Econometrica* Vol. 51, No. 3 (May, 1983), pp. 765-82.

⁸ McFadden, Daniel, “Contingent Valuation and Social Choice,” *American Journal of Agricultural Economics*, Vol. 76, No. 4 (Nov., 1994), pp. 689-708.

11. At his deposition, Professor Rausser conceded that he had not read the paper by Ayer et al. (1955),⁹ nor the books by Barlow et al.¹⁰ or Robertson et al.¹¹

12. Section 5.1 of the DR Report criticizes the ABERS estimator used by Chapman et al. The DR Report tests the robustness of the ABERS estimator by employing what it calls the Turnbull estimator, and it finds that the Turnbull estimator produces a lower estimate of mean WTP.

13. Section 5.3 of the DR Report states that the price and income elasticities implied by Chapman et al.'s non-parametric estimate of the WTP distribution are inconsistent with economic theory.

14. Both of these arguments are incorrect. They exhibit a lack of familiarity with the statistical and econometric literature on non-parametric estimation. They are not grounded in a correct scientific procedure, and they result from a limited and faulty understanding of the non-parametric methodology.

DR REPORT, SECTION 5.1

15. What the DR Report calls the Turnbull estimator is an estimator given that name in a book, Timothy C. Haab and Kenneth E. McConnell. *Valuing Environmental and Natural Resources: The Econometrics of Non-market Valuation*. Edward Elgar Publishing, 2002. The DR Report uses the procedure to calculate this estimator given by Haab and McConnell on page 69 of their book.

16. Anyone who is familiar with the literature on non-parametric estimation of binary response models (including models for single-bounded CV response data) would know that, in this particular context, the ABERS estimator is what Haab and McConnell call the Turnbull estimator. These are one and the same estimator.

17. Anyone who is familiar with the literature on non-parametric estimation of binary response models (including models for single-bounded CV response data) would know that the particular procedure stated on page 69 of Haab and McConnell's book is erroneous. It is an idiosyncratic procedure, not employed elsewhere in the peer-reviewed statistical or econometric literature on monotonicity-constrained maximum likelihood of a binary response model. It is inconsistent with the procedure that *is* described in the peer-reviewed statistical and econometric literature on this topic, including the literature cited in paragraph 10 above.

18. The attached spreadsheet illustrates the difference between the ABERS estimator and the estimator described on page 69 of the Haab and McConnell book. It presents some data which I have made up, intended as responses to a CV survey question like that presented by Chapman et al., involving five different bid amounts. Column A shows the bid amounts. Column B shows the percent of "yes" responses. There is non-monotonicity between the \$50 bid and the \$75 bid.

⁹ GR Deposition, page 130, line 3.

¹⁰ GR Deposition, page 130, line 8.

¹¹ GR Deposition, page 130, line 12.

The upper graph shows the ABERS estimate of the response proportions, corresponding to the proportions in column E. These correspond to the calculation presented by Morgan in his table on page 305. The lower graph shows the response proportions calculated according to the procedure given by Haab and McConnell on page 69 of their book, the corresponding proportions being given in column H. There is an important difference between the two graphs.

19. Haab and McConnell published a peer-reviewed journal article on this topic in 1997.¹² The journal article describes a procedure to calculate what it calls the Turnbull estimator that is identical to the ABERS procedure, and is crucially different from the procedure described on page 69 of the Haab and McConnell book.¹³ The procedure in the journal article corresponds to the upper graph in the spreadsheet mentioned in the previous paragraph while, as noted, the procedure in the book leads to the lower graph.

20. In their book, Haab and McConnell cite the 1997 journal article and purport to be following it. They present a proof of the derivation of the monotonicity-constrained maximum likelihood estimator of a binary response model, which is copied from the proof presented in their 1997 journal article. They then summarize on page 69 the estimation procedure following from this proof. However, the estimation procedure in the book does *not* follow from the proof that precedes it. It is an error.

21. I have pointed these facts out to Professor Haab, the author of the relevant chapter of the book. He responded in an email dated June 15, 2009, which is attached.

22. Professor Haab states: "I am in agreement that there is a discrepancy between Ted McConnell and my 1997 *Journal of Environmental Economics and Management* article and our 2002 book treatment of the Turnbull estimator for a lower bound on expected willingness to pay. Further, I'm in agreement that this difference is not trivial and leads to differences in the calculation of the lower bound on willingness to pay."

23. He agrees that the 2002 book treatment "is not mathematically correct" and does not correspond to the monotonicity-constrained non-parametric maximum likelihood estimator. He continues:

"So in my view, the Turnbull lower bound on expected willingness to pay treatment in the Haab and McConnell 1997 JEEM paper is correct and consistent with the original treatment as explained by Ayers, Morgan and Turnbull. In the second edition of the book (which is in slow progress), the following suggested corrections will be incorporated to ensure consistency between the 1997 paper explaining the Turnbull and the book.

1. p. 69, in paragraph before equation 3.16, delete and drop the $(j+1)$ th price.

¹² Haab, Timothy C. and Kenneth E. McConnell, "Referendum Models and Negative Willingness to Pay: Alternative Solutions," *Journal of Environmental Economics and Management*, Volume 32, 1997, pp. 251-70. Note that the Haab and McConnell book was *not* peer-reviewed.

¹³ Compare the text in the bottom five lines of page 256 and the top three lines of page 257 in the 1997 journal article with the text on page 69 of the book.

2. p. 69, under Procedure to Calculate the Turnbull, under step 4., delete eliminate bid t_{j+1} .
3. Table 3.5 on page 77: Instead of pooled back at the \$10 bid, enter .343 for the cdf and 0 for the pdf.
4. In the equation at the bottom of page 77, replace \$5 with \$10 so that .225 gets multiplied by \$10 rather than by \$5."

The four changes make the procedure conform to that described in the 1997 journal article and they generate the ABERS estimator employed by Chapman et al.

24. Professor Haab also confirms that what he calls the Turnbull estimator and what Chapman et al. call the ABERS estimator are one and the same.
25. In short, the DR Report lacks any grounding when it asserts a difference between the Turnbull estimator described in the Haab and McConnell book and the ABERS estimator employed by Chapman et al.
26. Similarly, the DR Report lacks any grounding when it criticizes the ABERS estimator used by Chapman et al. for being less conservative than the Turnbull estimator. The assertion is based on the difference between the upper graph and the lower graph discussed in paragraph 18. However, as Haab agrees, the lower graph is not mathematically correct.
27. At his deposition, Professor Rausser insisted that there is a distinction between the ABERS estimator and the Turnbull estimator:

15 Now, it's my understanding that the authors
 16 have argued that there is no difference in the
 17 Turnbull versus the ABERS with regard to estimating
 18 the cumulative distribution and, moreover, I
 19 listened in on some of the deposition testimony and
 20 I heard just such statements being made. That is
 21 true with regard to the cumulative distribution, but
 22 it's not true with regard to the derivation of the
 23 density function when anomalies take place, and
 24 that's why I used the Turnbull estimator of the
 25 willingness to pay off the density function. (page 134)

The deposition continues:

1 Q And did you rely on an excerpt of the Haab and
 2 McConnell book in your own work on that?
 3 A Yes, I did. (page 135)

In fact, as Professor Haab agrees, there is *no* difference between what Haab and McConnell call the Turnbull estimator and the ABERS estimator, either with regard to the cumulative distribution or the density function. Professor Rausser's statement in line 22 is simply incorrect. His understanding of the topic is narrowly rooted in the Haab and McConnell book. He is unfamiliar with their journal article or other important journal articles or books in the literature. Their book, as Professor Haab agrees, is in error. Thus, Professor Rausser's opinion on the Turnbull estimator is not grounded in a sound scientific methodology.

28. The DR Report states (p. 92):

"In describing its WTP estimation procedure, the Stratus report states: "... the estimated mean converges to the true mean of the distribution from below, meaning that the estimated mean underestimates the true mean in finite data sets." This statement wrongly implies that the ABERS WTP always underestimates the hypothetical "true" WTP, which is achieved as sample sizes approach infinity. This is only accurate when the original method of smoothing the cumulative distribution function is correct."

However, as explained above, the method of smoothing the cumulative distribution function employed by Chapman et al. corresponds to the monotonicity-constrained non-parametric maximum likelihood estimator, and therefore it is correct. The estimate of mean WTP calculated by Chapman et al. is the Kaplan Meier (1958) estimate of the mean for a non-parametric estimate of the WTP distribution. Kaplan and Meier prove that this converges to the true mean of the WTP distribution from below.¹⁴ Hence this statement on page 92 of the DR Report is not grounded in a sound methodology.

DR REPORT, SECTION 5.3

29. Section 5.3 of the DR Report discusses the elasticity of WTP in Chapman et al. In the case of the price elasticity, this "measures how responsive demand is to a price increase or decrease."¹⁵ The DR Report continues (page 100):

"Table 5.3 contains the elasticity calculations for both the base and scope versions of the Stratus survey. As the bid amounts increase, the percentage change in the bid amount is calculated. Similarly, the corresponding change in the percentage of respondents who vote in favor of the program (the quantity) is calculated. The comparison of these percent changes reflects the elasticity. When the bid goes up from \$10 to \$45, this represents a 350 percent price increase. However, in response to this price increase, the quantity demanded (represented by the probability of a "Yes" vote as reported by Stratus) falls only 14 percent, reflecting inelastic demand. Because all of the elasticity calculations are less than 1, the respondents' demand for improvements to the river and lake are inelastic. This result is inconsistent with expectations based upon the extensive literature previously described. Even more troubling, in the base survey result,

¹⁴ Kaplan, E. L., and Paul Meier, "Nonparametric Estimation from Incomplete Observations," *Journal of the American Statistical Association*, Vol. 53, No. 282 (June, 1958), pp. 457-481. Convergence from below follows from the analysis at the bottom of page 467 and the top of page 468.

¹⁵ DR Report, page 98.

the demand (probability of voting “Yes”) actually increases rather than declining in response to an increase in price (the Bid price). When the bid rises from \$80 to \$125 (a 60 percent price increase) the proportion of Respondents voting “Yes” to the expenditure simultaneously increases. Such results violate accepted economic tenets.”

30. The DR Report’s statement that “This result [an inelastic demand for improvement in rivers and lakes] is inconsistent with expectations based upon the extensive literature previously described” is puzzling. No such extensive literature is described. In fact, they cite *no literature at all* relating to the price elasticity of demand for a commodity such as improvement in rivers and lake. They cite one item in the preceding pages that relates to the price elasticity of demand. This is a paper by Scheierling, Loomis and Young (2006), cited on page 98 of the DR Report, and that is a review of the literature on the price elasticity of the *agricultural demand for irrigation water* as an *input* to agricultural production. This bears no conceivable relation to the household demand for water as a final good, let alone the household willingness to pay for improvement in rivers and lakes.

31. The other citations in pages 98-99 of the DR Report are to estimates of the *income* elasticity of WTP, as opposed to the price elasticity. Moreover, the studies cited by DR employ *parametric* models of the WTP distribution, not a non-parametric model like that employed by Chapman et al. This makes them fundamentally non-comparable to the Chapman et al. study.

32. The DR Report seems fundamentally unaware of distinction between a parametric model of demand (or willingness to pay) and a non-parametric model of demand (or willingness to pay). Parametric models place constraints on the shape of demand function (or the willingness to pay distribution) which typically ensure regularity properties, such as the property that demand is lower when the price is higher. Non-parametric models place no constraints on the shape of demand function (or the willingness to pay distribution); hence, there is no expectation that regularity properties will be satisfied in any finite empirical data set. The failure to satisfy the regularity property is an unsurprising consequence of non-parametric estimation.

33. This is illustrated in a well-known non-parametric study of the demand for gasoline in the US by Hausman and Newey (1995).¹⁶ Hausman and Newey apply nonparametric regression models to estimate demand curves for typical commodities. They employ two types of nonparametric estimator, a kernel estimator and a spline estimator. They apply these estimators to data on the household demand for gasoline in the US, using individual-level data from 18,109 households. Their data is thus comparable to the individual-level data used by Chapman et al..¹⁷ They estimate a total of six non-parametric models, which they compare with two parametric models estimated from the same data. They find as follows (p. 1461):

“There are interesting differences between the parametric and nonparametric estimates, with the nonparametric estimates having a much more complicated shape than the parametric ones. The kernel and spline estimates generally have a similar shape. There is some tendency to slope upward over a small range of prices.”

¹⁶ Hausman Jerry A., and Whitney K. Newey, “Nonparametric Estimation of Exact Consumers Surplus and Deadweight Loss” *Econometrica*, Vol. 63, No. 6 (Nov., 1995), pp. 1445-1476.

¹⁷ Because their data involve a continuous dependent variable rather than a binary dependent variable, as in Chapman et al., their non-parametric model takes a different form.

In fact, with all six non-parametric models there is some range of prices where a higher price leads to a *higher* rather than a lower level of demand (i.e., the demand function slopes upward).

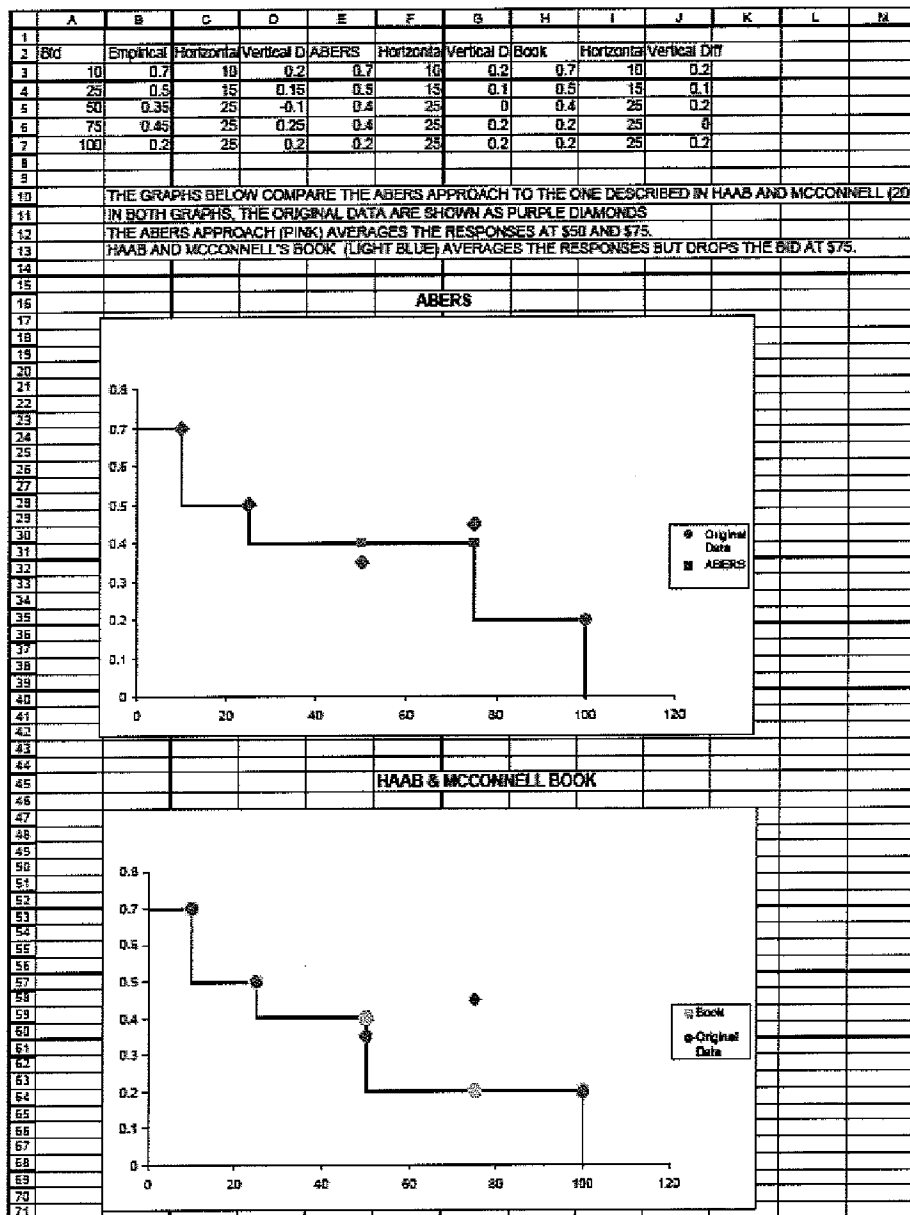
34. Hausman and Newey's finding that the demand curve slopes upward in some range of prices when estimated non-parametrically from individual-level data is no different than the result found by Chapman et al. The DR Report is mistaken when it characterizes this result as violation of accepted economic tenets. It is a natural consequence of non-parametric estimation with individual-level data.

35. The DR Report's assertions regarding the elasticities of WTP in Chapman et al. reflect a lack of understanding and/or experience with non-parametric estimation, and are not grounded in a sound scientific methodology.

ATTACHMENTS

1. SPREADSHEET ILLUSTRATING THE DIFFERENCE BETWEEN THE ABERS ESTIMATOR AND THE ESTIMATOR DESCRIBED IN HAAB & MCCONNELL BOOK.
2. EMAIL FROM PROFESSOR HAAB TO MICHAEL HANEMANN, JUNE 15, 2009

1. SPREADSHEET ILLUSTRATING THE DIFFERENCE BETWEEN THE ABERS ESTIMATOR AND THE ESTIMATOR DESCRIBED IN HAAB & MCCONNELL BOOK.



2. EMAIL FROM PROFESSOR HAAB TO MICHAEL HANEMANN, JUNE 15, 2009

Print

Page 1 of 2

From: Tim Haab ()
 To: Michael Hanemann; Barbara Kamminen
 Date: Monday, June 15, 2009 12:31:28 PM
 Cc: Ted McConnell
 Subject: Some comments

Michael,

I am in agreement that there is a discrepancy between Ted McConnell and my 1997 Journal of Environmental Economics and Management article and our 2002 book treatment of the Turnbull estimator for a lower bound on expected willingness to pay. Further, I'm in agreement that this difference is not trivial and leads to differences in the calculation of the lower bound on willingness to pay—as evidenced by the two different estimates of the Turnbull lower bound on expected willingness to pay for the Duffield data set (Page 77 in the book and page 260 in the article).

The difference in the two treatments stems from a point not addressed by any of the original papers since they were not concerned with integration of the resulting adjusted distribution for welfare estimates. The question boils down to whether the pooled bid still exists as a node of integration in the adjusted distribution for calculating welfare. In the 1997 JEEM paper, we assume that the pooled bid still exists and use it as a node of integration. In the book we eliminate it as a node of integration. Eliminating it results in a lower estimate of the lower bound on expected WTP. That explains the \$1.10 difference in the estimates for the Duffield data. But explaining the difference doesn't really give us any information other than we have to figure out which one is correct (and I'm sure this is where the center of the dispute lies).

My, perhaps limited, economic intuition tells me that we should eliminate the pooled bid for integration using the logic that if he said yes to a higher bid, then a rational individual would necessarily say yes to the next lowest bid and thus pooling all of those saying yes to the next lowest bid and eliminating the pooled bid as a node of integration imposes the least restrictive assumption on behavior we can make—resulting in the most conservative estimate of WTP.

This is the 2002 book treatment, which unfortunately is not mathematically correct. The constrained maximum likelihood estimate of the cdf must include the pooled bid as a node.

So in my view, the Turnbull lower bound on expected willingness to pay treatment in the Haab and McConnell 1997 JEEM paper is correct and consistent with the original treatment as explained by Ayers, Morgan and Turnbull. In the second edition of the book (which is in slow progress), the following suggested corrections will be incorporated to ensure consistency between the 1997 paper explaining the Turnbull and the book.

1. p. 69, in paragraph before equation 3.16, delete and drop the $(j+1)$ th price.
2. p. 69, under Procedure to Calculate the Turnbull, under step 4., delete eliminate bid t_{j+1} .
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4. In the equation at the bottom of page 77, replace \$5 with \$10 so that 225 gets multiplied by \$10 rather than by \$5.

Please let me know if you need any further clarifications.

Best Regards,

Tim Haab

<http://us.mg201.mail.yahoo.com/dc/launch?partner=sbc&gx=0&rand=dbpocotommsmrk> 6/18/2009

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Page 2 of 2

P.S. On your concern over the name of the estimator—while I would agree that initially the term Turnbull may have been misapplied to single bounded data (in my preliminary work for my dissertation I applied the estimator to single and double-bounded data and used the term Turnbull to cover both—but the double bounded work didn't appear in final versions), the 1997 JEEM article uses the term Turnbull and that seems to have become the accepted term among practitioners any dichotomous choice contingent valuation study. It's my view that trying to change the name to ABERS at this point would only lead to confusion among practitioners.

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<http://us.mg201.mail.yahoo.com/dc/launch?partner=sbc&gx=0&rand=dbpocotommsmrk> 6/18/2009

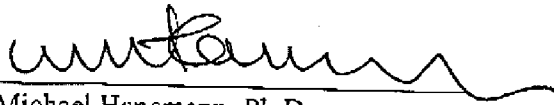
FROM : MICHAEL HANEMANN

FAX NO. :

Jun. 19 2009 10:41AM P2

I declare under penalty of perjury under the laws of the United States of America that the foregoing is true and correct.

Executed on June 9, 2009

A handwritten signature in black ink, appearing to read 'Michael Hanemann', written over a horizontal line.

Michael Hanemann, Ph.D.